MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXI. *June* 9, 1871. No. 8.

WILLIAM LASSELL, Esq., F.R.S., President, in the Chair.

John Brett, Esq., 6 Pump Court, Temple;
Rev. Reginald F. Dale, M.A., Queen's College, Oxford; and
Rev. Archibald Robertson, Burrage Road, Plumstead,

were balloted for, and duly elected Fellows of the Society.

The Variations of Gravity in the Western Provinces of Russia. By A. Sawitsch. (Abstract.)

A great arc of the meridian having been measured in Russia with all the precision which modern methods of observation will admit of, it became an interesting subject to examine the variations of the intensity of gravity in the districts traversed by this arc, and to compare the progress of those changes with the variations which are observed in the direction of gravity determined at several stations by astronomical observations and geodetical operations. An extensive series of pendulum observations was therefore arranged by the Academy of Sciences of St. Petersburg, to be made at certain stations between Tornea in Finland, and Ismail in Moldavia, selecting only those points of which the geographical positions and elevations above the mean level of the sea were determined in connexion with the great arc of meridian. The observations between Tornea and St. Petersburg were made during the summer of 1865 by M. Sawitsch and M. Lenz; those between St. Petersburg and Ismail were made in 1866 and 1868 by M. Sawitsch and M. Smyslof.

Two reversible pendulums were placed at the service of the observers. They were constructed by M. Repsold, of Hamburg. The pendulums differed from the English instruments both in their dimensions and in the arrangements of some of their parts. During the experiments, the clock and its tripod were inclosed in a case so that currents of air had no effect on the oscillations of the pendulum. A standard scale for the examination of the distance between the two knife-edges was also constructed by M. Repsold, and verified by M. Smyslof by comparison with the normal standard constructed by Messrs. Troughton and Simms, now deposited at the Observatory of Pulkowa.

M. Brauer, of St. Petersburg, furnished an apparatus for determining the centre of gravity of the pendulum, and also for the examinations of the parallelism of the knife-edges. M. Sawitsch remarks that these pendulums have now been placed at the disposal of Colonel Walker, Superintendent of the Great Trigonometrical Survey of India.

The observation of the duration of the oscillations of the pendulum consisted in noting the time of the coincidences of the pendulum and clock, the daily rate of the latter being deduced from comparisons of it with chronometers. A transit-instrument was erected on a brick pier, and the daily rate of the chronometers determined from transits of stars. Three or four coincidences and the temperature were observed at the beginning, in the middle, and at the end of each "swing." Three thermometers were placed in the interior of the pendulum-case, one at the upper end, another near the centre, and the third at the bottom end of the pendulum. The barometer was read at the beginning and end of each "swing."

M. Sawitsch gives in detail the formulæ used by him in the reductions, and also the results of the observations, which are given for twelve different stations situated between 65° 51′ and 45° 20′ north latitude. At St. Petersburg the length of the seconds' pendulum found directly from the observations is 440·958 Paris lines, the latitude being 59° 56′ 30″.

"Our object was not so much to determine the absolute length of the pendulum as to collect new data on the variations of gravity, and to compare them with those which have been found at other stations on the terrestrial surface. We know that at London the length of the simple seconds' pendulum has been determined with great precision by Capt. Kater and by General Sir E. Sabine; and that the measures made in Great Britain by M. Biot agree perfectly with those obtained by the English *savants*, while those made in France by the English *savants* give the same results as those of M. Biot. To these determinations we may also add the observations made by travellers and naval officers in different parts of the world. To connect our experiments with the preceding system of researches, without a breaking of continuity, we prefer not to adopt our direct determination of the length of the pendulum at St. Petersburg,

but rather that which was deduced from the oscillations of a similar invariable pendulum, observed by M. le Comte Luetke at St. Petersburg and at the Royal Observatory at Greenwich; the difference between the length of the seconds' pendulum at Greenwich and London having been accurately determined by Sir E. Sabine. Thus the length of the simple seconds' pendulum being known at London, we can calculate its length at St. Petersburg according to the relation of the squares of the numbers of infinitely small oscillations which the comparison pendulum, reduced to the same temperature *in vacuo*, has made at each of the stations in a mean day. In this manner, the calculation gives for the length of the simple seconds' pendulum at St. Petersburg 39·16975 English inches, or 441·0319 Paris lines. Assuming this length, we have deduced, as the result of our observations, the values contained in the following table."

Place of Observation.	Latitude N.	Longitude E. from Greenwich.	Length of the Seconds' Pendulum in Paris lines.
	° ′ ″	h m s	
Tornea	65 50 43	1 36 54	441·2525
Nicolaistadt	63 5 33	1 26 26	441·1293
St. Petersburg	59 56 30	2 1 14	441·0319
Réval	59 26 37	1 39 1	441·0190
Dorpat	58 22 47	1 46 54	440·9762
Jacobstadt	56 30 3	1 43 4	440·8900
Wilna	54 41 2	1 41 12	440·8353
Bélin	52 2 22	1 40 52	440·7268
Kréménetz	50 6 8	1 42 54	440·6533
Kaménetz-Podolsk	48 4 39	1 46 18	440·5844
Kischinef	47 1 30	1 55 18	440·5278
Ismail	45 20 34	1 55 16	440·4479

To examine the accuracy of each of the results, M. Sawitsch has compared the length of the pendulum observed at each station with the corresponding length obtained from the formulæ given in his paper. The residual errors are as follows: the sign + denoting that the observed length is greater than the calculated length.

	Paris Lines.		Paris Lines.
Tornea	+0 0200	Wilna	−0·0001
Nicolaistadt	−0·0141	Bélin	−0·0035
St. Petersburg	−0·0017	Kréménetz	+0·0017
Réval	+0·0033	Kaménetz-Podolsk	+0·0160
Dorpat	−0·0002	Kischinef	+0·0030
Jacobstadt	−0·0157	Ismail	−0·0071

The sum of the positive residual errors is +0·0440, and of the negative residual errors −0·0423.

Thus the formula agrees well with the equations of condition. With regard to individual errors, they depend upon errors of observation and upon anomalies in the intensities of terrestrial gravity; but it is difficult to discover in the differences given above any certain traces of these anomalies and of the local causes which produce them.

"In the work of W. Struve on the arc of meridian between the Danube and the Arctic Sea is a detailed discussion of the latitudes of the principal places between the North Cape and the Danube. The differences in latitude found directly from the astronomical observations vary only $\pm 1''{\cdot}75$ from those deduced from the geodetical operations. Although these differences are very much larger than the errors of observation, they are not as great as those which may be found in corresponding operations in other countries. Our stations are in the neighbourhood of the places discussed by M. Struve; so that it appears that in the great plains of Western Russia the directions and intensities of gravity are not subject to anomalies which change sensibly from one of our stations to another."

On the Physical Changes in Jupiter. By A. C. Ranyard, Esq.

It was suggested at one of the recent meetings of the Society, that if *Jupiter* had at any time been studied with sufficient optical power the same details would always have been apparent, and it was hinted that much which individual observers had been disposed to record as changes occurring upon the surface of *Jupiter*, was really to be attributed to the increased power of the instruments used.

In answer to such objections I propose to compare a small series of drawings, all made within the last twenty years, by well-known observers and with large instruments.

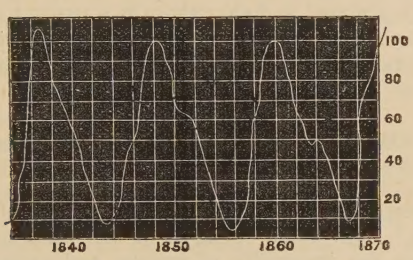
We will commence with a drawing by Mr. Lassell, a very poor woodcut from which is given at page 134 of vol. x. of the *Monthly Notices*. It was taken with his 20-feet reflector and 24 inches aperture. *Jupiter* is there represented within two years of the great Sun-spot maximum of 1848, and the strongly-marked white spots in the southern belts, and the broken condition of the equatorial region, are very apparent.

Within a year of the next Sun-spot minimum Mr. W. De La Rue made his large and well-known drawing of *Jupiter* with a 13-inch silver speculum. Its date is October 1856, and his engraving, which was largely circulated, was at that time universally acknowledged to be a very just representation of the state of the planet. It is full of the smallest details in the belts, yet there are no traces to be found of Dawes' markings, bright points, northern or southern eggs, or equatorial port-holes.

A second drawing, made in 1856 by Piazzi Smyth on *Teneriffe* with a $7\frac{1}{4}$-inch refractor, when *Jupiter* was near the zenith,

entirely endorses that of Mr. W. De La Rue. The next Sun-spot maximum occurred at the end of 1859; and in Nov. 1858, Mr. Lassell again noticed and figured the Dawes' markings and white spots. He says, at page 52 of the *Monthly Notices*, vol. xix., "One of the most novel features I noticed was the existence of a numerous group of white spots in the bright equatorial region, far more delicate and difficult to see than those in the southern hemisphere, which I first noticed in March 1850. For several years I failed to see any such spots upon the face of *Jupiter* at all, but last year they appeared again in the same quarter of the planet, and were attentively and ably observed by Mr. Dawes."

In September 1859 the flocculent cloudy port-holes in the principal belts made their appearance and were beautifully drawn by Sir J. Keith Murray with his 9-inch refractor. They remained more or less a marked feature of the central belt till the end of 1861, and were figured by many observers with telescopes varying from 5 inches in aperture and upwards — though they are not to be found in a very elaborate series of drawings made by Mr. Gorton with a 3½-inch Cook in the years 1860–1861.



I cannot here omit to quote a remark made by the Astronomer Royal in his Report to the Visitors of the Greenwich Observatory for the year 1860–61. Speaking of the great Equatoreal, he says, 'It has been employed to a considerable extent in the preparation of delineations of *Jupiter* and *Mars*. The former of these planets has exhibited in the last year some appearances never before recorded; and it has appeared very desirable to register as soon as possible anything which seems to indicate a change in the constitution of that great body."

Mr. Carpenter during that year made a series of most careful drawings, showing all the flocculent port-holes and reddish colour of the equatorial region, bright eggs, and elliptic markings, which have been so noticeable during the last two years.

The next minimum of Sun-spots occurred early in 1866.

I have been able to find very few drawings of *Jupiter* in or near to this year, and my conclusion is, that there must have been but few conspicuous bright or flocculent markings to attract attention.

The Rev. T. W. Webb has been so good as to examine his

observatory note-books for drawings of *Jupiter* from 1863 to 1867. He only finds two drawings during this period with anything like white egg markings; the first on June 23, 1863, and the second in August 1866, with two white spots; one in a southern, the other in a northern belt; but he thinks that they have evidently nothing to do with the egg markings of 1860–1870.

It is unnecessary here to give any account of the reappearance of the same phenomena during the recent Sun-spot maximum. I append a Sun-spot curve for the period 1834–1870.

On account of the interest attached to the question of the variability of the Nebula of *η Argûs*, the Council have determined to print as well Mr. Abbott's communications as the remarks upon them by the late Sir J. F. W. Herschel, and the Astronomer Royal. For convenience of reference it may be mentioned that Mr. Abbott's former papers are printed in the *Monthly Notices*, vol. xxi. p. 230 (June 1861); vol. xxiv. p. 2 (November 1863), with plate; vol. xxv. p. 192 (April 1865, paper dated 18th February); and vol. xxviii. p. 200 (May 1868, paper dated 29th February), with a plate; and that there is a paper by Sir J. F. W. Herschel, vol. xxviii. p. 225 (June 1868); and one by him and Lieut. Herschel, vol. xxix. p. 82 (January 1869), with five plates.—Ed.

Some further Observations on the Variable Star η Argûs, and its Surrounding Nebula. By F. Abbott, Esq.

I now forward to the Society, after a period of two years, a third drawing,* with some remarks in reference to more recent observations on the inequality of motion, and variation in aspect of *η Argûs*, and the surrounding Nebula.

On my last communication some critical remarks were made, with a view of disproving that which I never intended to prove. This, in all probability, arose from my not having expressed myself with sufficient clearness in my remarks on the drawing which accompanied the observations. In this way the road to truth often runs through the midst of error, but that does not in any way alter the fact that great changes have been, and still are, taking place in the object under consideration.

Perhaps the field given in the drawing of 1868 may be somewhat too large, as it was my first intention to have made it larger; but, finding that the changes were principally confined to η and the so-called Lemniscate,† I confined the drawing to the size of the field given with the eye-piece.

* See plate of *η Argûs* as taken Jan. 28, 1870.—Ed.

† I scarcely think this term a good one:—Lemniscate, or Lemniscus; a curve formed as the figure 8, or a bow tied of a riband—Barlow and B. H. Smart. Such a curve is closed in the centre, which is not the case in the Cape drawing, it being there shown as a long enclosure slightly compressed in the centre. It was in this compressed part that the star η appeared when out of the dense Nebula.

As some objections have been raised on the ground that larger optical means than those employed by me were required for recording truthfully the changes which take place in this object, it may be well, perhaps, to state that the 5-feet Equatoreal previously mentioned is not the only instrument employed. Other telescopes have been used in the open air, from a 3½-feet Cook and Sons, to a 7-feet Dollond, with, for the whole, a complete battery of micrometers and eye-pieces, giving magnifying powers of from 25 to 450. This statement may go towards proving that observations can be correctly made without very large instruments.

In Mr. Proctor's article on the Nebula in *Argo* (*Fraser's Magazine* for December 1868) it is stated not to be quite clear that the stars which appear in my drawing of 1868 have been really copied from the view given by the telescope, &c. In reply to this, I beg to remark that all the drawings, the present as well as the former ones, were carefully copied from the object, as described in the *Astronomical Register* for January 1869. There is little doubt but that Mr. Proctor's views on this subject would be much enlarged if he had the opportunity of seeing the star and Nebula as they appear in the telescope, when above the Pole, at Hobart Town.

By these means, and having seen the object in the Melbourne reflector, I feel armed with sufficient courage to communicate what I have observed regarding the changes which have taken place in this object during the last two years.

In comparing the present drawing with that of 1868, it will appear clear that alterations have taken place, both in the magnitude of the star η and in the dispersion of the Nebula; and from what follows it will be seen that the remarks made by Professor Loomis and others on the period of this star are premature. When in Melbourne in June last, and while observing the object, Mr. Ellery considered the star η to be of the seventh magnitude, and Mr. Le Sueur thought it 6½. On my return to Hobart Town, I continued to observe it, and from careful comparisons made with the stars given in the drawing, and recorded in the Cape Catalogue, it cannot be of more than the seventh magnitude. In the Cape Catalogue there are two stars of the sixth, and nine of the seventh magnitude; the remainder are all low magnitude stars. The two sixth magnitude stars are out of the field. With the nine seventh magnitude stars in the field, they are by careful comparison exactly the magnitude of η, which is left amongst them not marked.

The magnitudes of these stars are those given by Sir J. Herschel, and may be considered correct.

Measures are recorded to have been made of these stars by small transit means; but, from my own experience in transit observations, I believe it to be all but impossible to measure correctly such a cluster of small zenith stars by such means. Two years ago I dismounted a 24-inches transit by Varley, in order to replace it with a 30-inches by Dallmeyer, made with a deep dia-

gonal eye-piece, for the purpose of reaching small zenith stars up to the seventh magnitude, as agreed upon for correcting any error in longitude between Hobart Town and Melbourne. Mr. Ellery selected forty-nine such stars, which were to be used at both places, reversing the instruments at each observation so as to eliminate any errors. With these means, and for this purpose, I find it difficult, and only under very favourable circumstances possible, to reach stars of the seventh magnitude.

My reason for not attempting measures with the Equatoreal is, in consequence of a long-experienced difficulty arising from the want of clock movement, which I consider indispensable for the accurate measurement of distances. I preferred, therefore, an eye-and-hand drawing, when the object has been in a convenient position, approximately 75° from the meridian towards the east, and 35° from the zenith.

The alterations which have taken place in the Nebula since 1868 will at once be seen on an inspection of the drawings, and by comparing them with each other. In the Cape Monograph, the dark space is an inclosure. In 1863 it had two openings, one at each end. In 1868 there were four openings; and now, in 1870, there are five which expose a number of isolated and distinct stars, rendering it difficult, but from position, to know which is the star *η Argûs*. There is attendant on these changes an increase of light which is notable up to the present time. During the last three full Moons (December, January, and February), when both the object and the Moon are approaching the meridian, the light around *η Argûs* is distinctly seen when all other Nebulæ are shut out. This increase of light may arise from one of two causes; either the Nebula has become concreted into isolated stars, or in its dispersion it has laid bare distinct stars which give out more light. The same physical forces that have worked out our own solar system are still at work in the stellar universe. Creation is still going on, and why not?

Private Observatory, Hobart Town,
Tasmania, Feb. 15th, 1870.

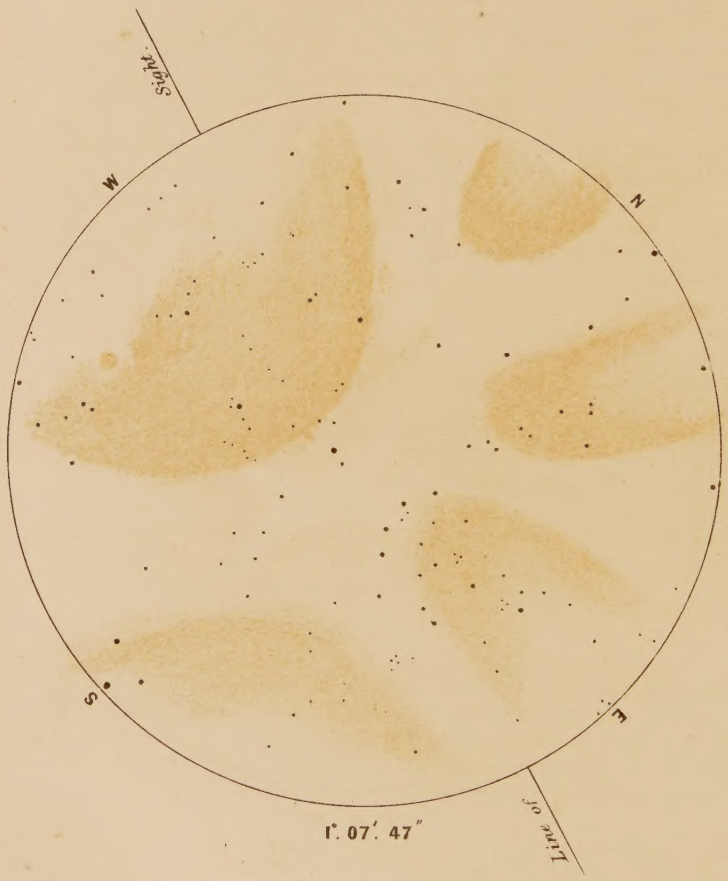
P.S.—I may mention, in conclusion, that I have on several fine nights tried one of Mr. Huggins' hand spectrum telescopes, as described in the *Proceedings of the Royal Society*, vol. xvi. No. 98, without being able to detect any appearance of bright lines in the Nebula of *η Argûs*, such as those seen in that of *Orion*.

Remarks on Mr. Abbott's foregoing Paper on η Argûs.
By Sir J. F. W. Herschel, Bart.

Pursuant to the request of the Council of the Royal Astronomical Society, I have carefully perused Mr. Abbott's communication on *η Argûs*, dated Feb. 15, 1870, and examined the dia-

η ARGUS AS SITUATED TO THE DARK SPACE,
VARIABILITY OF THE SURROUNDING NEBULÆ,
WITH THE MAGNITUDE AND POSITION OF ACCOMPANING STARS.
UP TO THE PRESENT TIME.

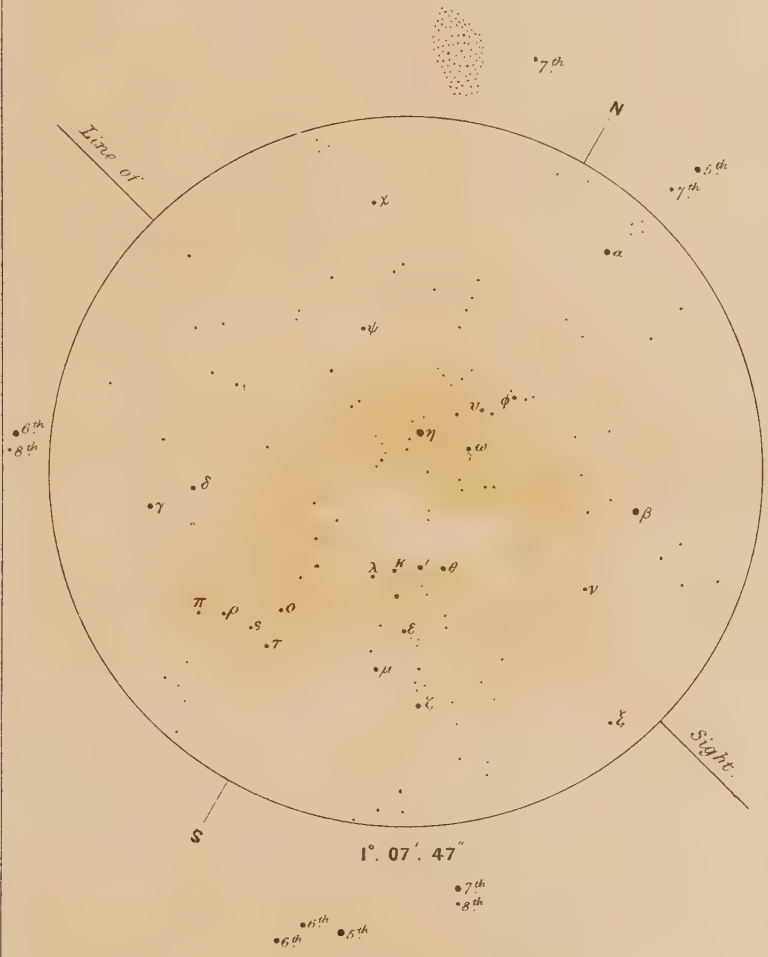
1870.



TAKEN JANUARY 28TH 1870.

W. Metcalfe & Sons, Litho. Cambridge.

APPROXIMATE DISTANCES AND MAGNITUDES OF THE PRINCIPAL STARS AND NEBULÆ SURROUNDING η ARGUS. TAKEN AT HOBART TOWN, FEBRUARY 1871.



W. Metcalfe & Sons
Lithe: Cambridge

gram accompanying it. Comparing the latter with Mr. Abbott's former representations of the Nebula, viz., that for 1863, in vol. xxiv. p. 5 (*Monthly Notices*), and that for 1868, in vol. xxviii. p. 200, the amount of variation is so astonishing that, considering all three to have been figured by the same observer, using (at least for the two last) the same telescope (a 5-feet Equatoreal, aperture not stated), it would seem to be doing injustice to Mr. Abbott not to place this last drawing also on record in the same manner as the others.

At the same time there is this difficulty. On comparing the present diagram with that in vol. xxviii. p. 200, it will be seen that, as regards most of the principal *stars* laid down, and several of the smaller groups, they exhibit on the whole a sufficient general agreement, considered as eye-drafts, while both are utterly irreconcilable both with my son's configurations (*Monthly Notices*, vol. xxix. p. 85), and with my own recorded observations, with which the latter entirely agree. On carefully examining the *present* diagram, I find it open to the very same remarks which I had occasion to make in my former communication on the same subject, viz., that there is not one among all the stars delineated which I can identify with any of those laid down in my own drawings and catalogued positions. The orientation (which I had surmised might possibly have been mistaken by Mr. Abbott in copying, or by myself), being here placed, by its reiteration, beyond a doubt; the most superficial inspection suffices to show that there is no correspondence whatever between us; and that his field of view of 1° 8′ diameter differs as completely from a similar field, in my monograph having *η Argûs* near the centre, as if the telescope had been directed to quite a different part of the heavens. For example, Mr. Abbott places within a distance of 11½′ (on the scale of his drawing) from *η*, five stars of magnitudes at least equal to *η*, that is, 7 mag. (if I am right in supposing the large star a little south-preceding the centre of his figure to be intended for *η*, and according to his former figure it can be no other), while in my monograph only one star of that magnitude (marked O) occurs within that distance; and five of the 8th magnitude, F, D, D′, C, K′, which however stand quite differently in relation to *η* from any of the larger ones in Mr. Abbott's diagram.

This would, however, be of little moment, were it permitted to suppose that attention had been given only to the delineation of the Nebula, and that the stars had been put down at random or with little regard to their real configurations. Mr. Abbott, however, in the paper which accompanies his diagram, distinctly repudiates this supposition, and insists on the correctness of his representations of the stars in the field of view delineated; not, indeed, as micrometrically accurate, but as careful eye-drafts. "All the drawings," he says, in reference to this especial point, "the present as well as the former ones, were carefully copied from the object."

Under these circumstances, were Mr. Abbott resident in England, I should feel disposed to recommend referring the communication back to him (*accompanied with these remarks*) for such elucidation of the causes of the discrepancy in question as the case may admit, and with a request that he would furnish *some* instrumental determination (however rough) of the differences of R.A. and P.D. between η and the eight (or nine ?) other stars of equal magnitude with η, which he states to be included in his diagram, as also the *apertures* of the telescopes used; and, deferring the final decision of the Council as to the publication *in extenso* of the communication, to notice in the forthcoming Number of the *Monthly Notices* of proceedings, the fact of its having been read, with mention of his having observed on January 28, 1870, the Nebula to consist of five nebulous arcs or masses convex towards each other and towards η, which occupied a position at or near the point of embranchment of the same number of non-nebulous roads or channels running outwards in five different directions.

The Council, however, will best judge whether such a course is advisable, taking into consideration the length of time which must elapse before a reply could be received from Hobart Town.

Collingwood, May 19, 1870.

The substance of these remarks was communicated to Mr. Abbott in a letter dated 13th June, 1870, and he replied thereto in the following communication:—

In reply to the queries set forth in the letter of the 13th June last, I may state that it is not my intention or desire to dispute either Sir John's or Lieut. Herschel's configurations of the star η *Argûs* and its Nebula, but to draw the attention of the astronomical world to the altered features of both, with a view of ascertaining a solution of the changes seen in this most remarkable object.

My own opinion is, that no two widely different instruments will show the gradations of nebulous objects exactly alike; it will require a standard instrument. In my own case I have used the 5 feet with a 4-inch clear aperture, a comet eye-piece of 28, both perfect, for twelve years. And as a check, in the open air, I use a $3\frac{1}{2}$-inch Cook and Sons, with a Kellner's orthoscopic eye-piece of 50.

Most of the questions put to me have been explained in former papers, excepting as regards measurement. In this I am not at all inclined to dispute either Sir John's or Lieut. Herschel's statements; what I contend for is the diminution in the size of the star η, and a continued, rapid breaking up and displacement of the Nebula surrounding it; both sufficiently apparent to any one acquainted with the object, and to be seen on fine nights with the naked eye.

The last question you require to be answered is, "Can I give any elucidation as to the difference?" &c. This borders on a physical question, and on that point I regret to have caused some displeasure to Sir John Herschel, by quoting a passage in the fifth edition of his *Outlines*, p. 639, sec. 871.

The object is now getting below the Pole, and daylight fast increasing; but when a favourable opportunity occurs, I will not omit to pay every possible attention to the requirements contained in your letter, feeling convinced, notwithstanding the contrary opinion, that the fluctuations which have taken place in *Argus* will some day become better known.

With regard to the fluctuations, distance and position very materially affect visible motion, and the changes about η are physical, if any, and therefore it would be necessary to adopt a real standard instrument, such as the old 18-inch erected at the Cape of Good Hope in its former position.

P.S.—I cannot refrain from mentioning my surprise at seeing an extract from a letter to the Astronomer Royal from Mr. H. A. Severn, of Melbourne, in the *Monthly Notices*, vol. xxx. p. 180. It is utterly at variance with the author's letters addressed to me, in all of which he not only acknowledges the changes in *Argus*, but also appears anxious that I should be credited with the discovery.

Surely there must be some mistake in the extract from the *Notices*. I have this moment received another letter from Mr. Severn, in which he expresses the same opinion on the fluctuations as before.

Private Observatory, Hobart Town,
Tasmania, 7th September, 1870.

There has since been received from him the following further communication:—

On η Argûs and its Surrounding Nebula. By F. Abbott, Esq.

To the Honorary Secretaries and Referees appointed by the Council of the Royal Astronomical Society to make inquiry as to certain alterations which have taken place in the star η *Argûs* and its surrounding Nebula.

In carefully looking over the drawings referred to, which were taken at Bangalore by Lieut. Herschel, with the object η *Argûs* 15° above the horizon, and also the *reversed* copy of Sir J. Herschel's, and on consideration of the discussion given with the drawings, I do not think that Lieut. Herschel's observations tend to disprove any one of the alterations which I have previously communicated to the Society, but, on the contrary, rather to confirm them. This, I think, will appear clear by an inspection of

the present drawing* and the answers given to questions put by the referees.

The present drawing is taken with the same instrument as the former ones, the object in the same position approximately 80° above the horizon. The measures have been made with a *bar micrometer* by Cook and Sons, the bars being carefully traced in pencil on the drawing paper in such a way as to exactly fill the field of the telescope. All the stars visible were then dotted down, the distances from η of the sixth, seventh, and eighth magnitude stars were lettered, measured, and catalogued from a scale of equal parts; after which the micrometer pencil lines were rubbed out, and the Nebula inserted.

The first question put by the referees relates to a comparison of the positions of the principal stars and smaller groups, as shown in my two drawings, which are said to have a sufficient general agreement with each other, considered as eye-drafts, while they are irreconcilable with both Sir John and Lieut. Herschel's configurations. A simple inspection of my drawing of 1870, with the reversed drawing (A A), plate 4, of Sir J. Herschel, in the *Monthly Notices*, will show that the following principal stars hold a relative position, considered as eye-drafts, but not with the Cape monograph, as expressed in the letter D′, D—C′, C—(ν), ι—B—ζ—(ε)—522—558—640—337—383—415—(γ)—(λ), &c. &c. There are many other stars in my copy of 1870 that are not laid down in plate 4, pricked off from Lieut. Herschel's drawing.

The other question of note refers to my "having placed within 11½′ (on the scale of my drawing of η) five stars of magnitude at least equal to η, that is the 7th mag., while in Sir J. Herschel's monograph only one star of that magnitude (marked C) occurs within that distance, and five of the eighth mag.," and continues, "Can you give any elucidation of the cause of the discrepancy? also, if you would furnish some instrumental determination of the difference of R.A. and P.D. between η and other stars of equal magnitude."

In my acknowledgment of this letter to Mr. Wm. Huggins,

* Plate of η *Argûs*, as taken February 1871.—Ed. The drawing has upon it a reference to the several stars, as follows: —

	Mag.	Dist. from η.		Mag.	Dist. from η.		Mag.	Dist. from η.
		′ ″			′ ″			′ ″
α	5	29 30	ι	7½	15 45	ϱ	8½	30 20
β	6	26 0	κ	7½	16 10	σ	8½	29 15
γ	6½	32 0	λ	7½	17 30	τ	8½	29 40
δ	6½	26 30	μ	8	21 25	υ	8½	8 20
ε	7	22 30	ν	8	25 50	φ	8	12 0
ζ	7	31 20	ξ	8	40 0	χ	8½	26 40
η	7	—	ο	8	25 30	ψ	8½	13 50
θ	7½	16 15	π	8½	32 25	ω	8½	6 0

F.R.S., &c., I mentioned that it was not my intention or desire to dispute either Sir John's or Lieut. Herschel's configurations, but to call the attention of the astronomical world to the altered features of both the star and the nebula, with a view of obtaining a solution of the changes seen in this most remarkable object. I further stated that the above question was of a physical nature, and could only be answered as such.

On reference to my former papers it will be seen that mention is made, more than once, of the fact that the increase of stars of the same magnitude as η renders it difficult to know that star from others only by its position and a marked difference in the *light*. The present drawing will show a still greater and more remarkable number of stars of a similar magnitude.

It is to this cause I have so frequently referred, the increase of light, which I think is now clearly confirmed by both Sir John and Lieut. Herschel. At one of the monthly meetings of the Society Sir John considered the increased light in the object, as recorded, very strange, and remarked, "When I was at the Cape the Nebula could not be seen at all with the naked eye." Lieut. Herschel, at Bangalore, compared the light when the object was 15° above the horizon to *Pleiades* in *Taurus*.

Mr. Le Sueur, in his report on the Melbourne Reflector, says, "that the Nebula around η *Argûs* has changed largely in shape since Sir J. Herschel was at the Cape. The star shines with the light of burning hydrogen," and in his opinion "has consumed the Nebula."

The Catalogue will show that there are now in the same field two stars of the 6th, two $6\frac{1}{2}$, three 7th, four $7\frac{1}{2}$, four 8th, and nine of the $8\frac{1}{2}$ magnitude, and it is literally crowded with others of from the $8\frac{1}{2}$ to the 12th magnitude. Those lying outside the field and occupying an area of about $1\frac{1}{2}°$ have their magnitudes attached. The small cluster I take to be Sir J. Herschel's 3276, described as a "fine, bright, rich, not very large cluster;" if so, it is now a beautiful cluster of richly coloured stars, quite equal to χ *Crucis*.

It is almost impossible to define the boundary of the Nebula as it appears to be gradually fading away, and is not so distinct in outline as formerly.

The finest nights have always been selected for observing, and no delineation of the object has ever been given but what was an accurate representation of its appearance through the telescope.

Private Observatory, Hobart Town,
Tasmania, 17th February, 1871.

The foregoing Papers were referred to Mr. Airy, who remarks upon them as follows: —

I duly received the packet of papers relating to Mr. Abbott's observations on η *Argûs;* and with these I have perused

also the preceding papers in various volumes of the *Monthly Notices*. The subject is really a very puzzling one.

1. As regards stars only, the map of 1870 and the map of 1871* have so much difference (not a great deal) that I conceive them to be certainly independent; and yet they have so much similarity as to give strong probability to their faithful representation of the visible objects. See in particular the line of four stars convex towards *η Argûs*.

2. These four stars have some agreement, not quite good, with four of Sir J. Herschel's. But other stars in the concavity of the bend are wanting in Sir J. Herschel's.

3. When we look more closely to fundamental points, all is confusion. Mr. Abbott's observations were all made with a refracting telescope: in that case the order of the four cardinal directions on the map would be $E \begin{matrix} N \\ S \end{matrix} W$ (turned round, in any degree). But in the 1870 map he has marked them $W \begin{matrix} N \\ S \end{matrix} E$: in the 1871 map he has marked only $\begin{matrix} N \\ S \end{matrix}$. The $\begin{matrix} N \\ S \end{matrix}$ in the two maps differ about 45°.

4. In each of these maps is a line (different for the two) which he calls "Line of Sight." What this means I have not the slightest idea.

5. In points of geometry, therefore, Mr. Abbott is a most inaccurate man.

6. It is impossible for us to publish maps in this state.

7. As regards the delineation of the nebula, I cannot make out anything. The drawing in the second map, dated February 1871, agrees EXACTLY with Lieut. Herschel's drawing, 1868, Nov. 23, published in the *Monthly Notices*, 1869, Jan. 8 [misprinted 1868 in the *Monthly Notice*]. This drawing (Lieut. Herschel's) had undoubtedly reached Australia. Has Mr. Abbott copied it? Mr. Abbott's first map, dated 1870, Jan. 28, is irreconcilable with Lieut. Herschel's, which preceded, or with his own which followed.

8. While, therefore, I do concede to Mr. Abbott the merit of first pointing out that the nebula has shifted its position with regard to the star *η Argûs*, and has changed its form materially (both which points I regard as certain), I feel most strongly that his maps are utterly unfit for publication.

Allow me to suggest that papers of this kind ought to be preserved more carefully. Mr. Abbott's maps were crimped up till I could not read them. I have flattened them, and will endeavour to persuade Mr. Dunkin to keep them flat.

Royal Observatory, Greenwich,
1871, *May* 25.

* See the two Plates of *η Argûs*.—ED.

On the Nebula of η Argûs.

(*Letter from Lieut. Herschel to J. F. W. Herschel, Bart.*)

I hope I have not wasted time in examining Mr. Abbott's sketch of Nebula *Argûs*. It is only by accident that I see the *Monthly Notices of the R.A.S.*, so when at the Madras Observatory lately I took a tracing which I have here pricked off to send you, because I find that the stars marked are not so unidentifiable as I supposed. I have lettered a few; the others will, I dare say, be recognised as soon as the clue is given. On the whole, they appear to be not very incorrectly placed.

It is evident, I think, that in making this drawing a low power was used, which failed to show such details as the lemniscate. The broad outlines are not very dissimilar from my low-power sketch (No. 1, *Monthly Notices*, Jan. 8, 1869).

Mr. Abbott appears to have got wrong in his N. and S. points, if I have traced correctly. Is it rash to suspect that he has also mistaken η? In a letter to the editor of the *Astron. Register* (V. No. 73), he speaks of it "leaving the thick part and fixing itself in the dark space." It is not so in his own drawing, and it certainly is not so in reality, if my drawings are worth anything at all. Neither does he appear to have so much as recognised the lemniscate. He also speaks of "a dispersion of the stars," but his own drawing, as I now show, places most of his stars in approximately their right relative places. Surely all this betokens *non-recognition* on his part of the object he was examining, due probably to an inferior magnifying power.

If his chart of the stars is as correct as I think (I have no copy but this hasty trace at hand, and cannot find your engravings, unfortunately) every atom of evidence of change in the *nebula* which *he* adduces is swept away, because he at the same time testifies to a "scattering of the stars," which his own drawing proves to be a delusion.

As for the non-interpretation of his chart, the truth seems to be that a low-power drawing has been looked at as if it were a small portion full of detail. You speak (in your letter to the late Admiral Manners) of the "breaking up of the well-defined southern border by two clear open channels, which forms so marked a feature in Mr. Abbott's delineation." I think you will now see that the latter is on too small a scale to show the lemniscate, at any rate that there is no trace of it, and that the "channels" are distinct and real features in another part of the nebula.

If this mystery is now cleared up, it will be well to hesitate in accepting the apparent changes shown in *my* drawings as well. They will probably be shown, under higher power and increased light concentration, to be likewise mistaken attempts to show details insufficiently clear.

Bangalore, May 7th, 1871.

A Proposal for a series of Systematic Surveys of the Star-Depths. By R. A. Proctor, B.A. (Cambridge).

When we consider the vast addition to our knowledge, and the yet vaster widening of our conceptions respecting the star-depths, which resulted from the labours of Sir William Herschel and the great man whose death science is now deploring, we cannot doubt that more complete surveys, if such could be carried out, would well repay the pains bestowed upon them. I apprehend that not the least among the purposes which the elder Herschel proposed to fulfil when he commenced that first great survey was to show astronomers how much the survey of the heavens was needed; and I imagine that he would have been the last to approve of that supineness (arising, perhaps, from an exaggerated respect for his labours) which has prevented all save his son from pursuing the path which he was the first to indicate. As to the opinion of Sir John Herschel on this matter, it is unnecessary that I should speak; because he has in many parts of his works urged in his own earnest manner how desirable it is that the celestial depths should be studied much more closely than they could be by only two observers, however skilful and energetic, or however patiently they continued their labours for many successive years.

I believe that what is now specially required is a series of systematic surveys, proceeding on a principle quite different from that on which the Herschels found it necessary to pursue *their* researches. A first survey was very properly, or rather, it was necessarily applied with reference rather to the average distribution of the stars than to the special laws of distribution which may be found to prevail either when we extend our survey over the different parts of the celestial sphere, or when we vary the range of our vision by employing different telescopic powers.

I venture now to impress most earnestly on those who have sufficient leisure and possess the necessary instrumental means for carrying out systematic observation, the extreme importance of surveys of the star-depths on methods devised with reference to both these relations.

Let me briefly indicate what is at present wanted, noting at the same time, that the proposed observations should only be regarded as first steps in a progressive series of researches directed to the solution of the noblest of all the problems astronomers can deal with,—the determination of the laws (so far as they are discoverable) according to which the heavens are constituted.

(1.) I have already mapped down isographically the stars visible to the unaided eye; and I think that no one can study my isographic chart, or the numerical statistics which accompany it in the second edition of my *Other Worlds*, without feeling that

even what the heavens disclose to the unaided eye has a significance which has too long been suffered to escape recognition.

(2.) I am engaged in mapping down isographically the stars in Argelander's charts of the northern heavens — 324,000 stars in all, about 310,000 north of the equator, and about 14,000 within two degrees south of the equator. When this chart is completed, it will serve to show, so far as the northern heavens are concerned, what are the laws of distribution among those stars which lie within the range of a telescope $2\frac{1}{4}$-inches in aperture. The correspondence between these laws in certain parts (at least) of the heavens, with those observed in the distribution of naked-eye stars on the one hand, or of the Milky Way, nebulæ, &c., on the other, will throw light on many questions of interest,—for instance, on the numerical relations of large and small stars (stars really large and small, that is), and (consequently) on the evidence as to the distances of stars of different apparent orders. Light will also be thrown on many other questions of importance. That this is so I shall be able (I already know from the progress of the work) to establish; but at present it will suffice to notice, as evidence of the importance of such researches, that Struve, taking only a small portion only of Argelander's charts, and dealing with that portion only by averages,* not only as respects extension in right ascension and declination, but also as respects probable extension in space, was yet led to results of extreme interest if admitted, and highly suggestive even if regarded as still open to question.

The system of charting with reference to an aperture of $2\frac{1}{4}$-in. requires extension over the whole southern hemisphere. The results of this extension alone would be (I venture to predict) of the utmost possible interest. For the purpose of increasing our knowledge of the constitution of the star-depths, actual charting would not be needed, but only such a process of statistical enumeration as I propose in the case of larger apertures.

(3.) The Council of this Society have kindly placed at my disposal the Sheepshanks Equatoreal No. 3, having an aperture of $4\frac{1}{2}$-in., and I propose to apply this instrument to the enumeration of stars lying within the increased range belonging to its aperture. The plan I propose to follow is to count the number of stars seen in equal fields taken all over each region surveyed, and then to map down the result isographically. It is probable that the very limited amount of leisure time I can devote to such researches may prevent me from surveying more than a small region of the northern heavens. But as my special object is to show what lessons such surveys are calculated to teach, I shall be well satisfied if I can thus, by example more effectively than by precept, engage others who have more leisure and instruments of

* How rough these were (intentionally, of course) is shown by the fact that the charted results did not even indicate the division of the Milky Way into two parts over a region crossed by Struve's section. "C'est une suite naturelle," says Struve on this point, "de ce que notre recherche du disque ne s'est point faite dans tous les détails, mais *par heures entières* d'ascension droite."

about the size named to extend this survey to much larger regions. If the results are not charted by the observer himself he should keep a gauge-book from which an isographic chart could be formed hereafter.

The southern heavens also should be surveyed with a telescope of about $4\frac{1}{2}$-in. in aperture.

(4.) A survey on a similar plan should be carried out over the whole heavens with a telescope 9-in. in aperture.*

(5.) Star-gauging with powers about equal to those of the telescopes with which the Herschels surveyed the heavens, should be carried out on a much more complete plan than was possible in the case of astronomers engaged like the Herschels on many different branches of research simultaneously. In the present position of sidereal astronomy, more is to be gained by the complete survey even of a small region in this way (followed by careful isographical charting) than by an incomplete survey depending on a law of averages which has been proved *not* to prevail in stellar distribution.

(6.) A good 4-ft. speculum should be applied to the same purpose, even though half-a-dozen generations might have to be occupied in the work.

Feeling absolutely certain of the extreme interest and importance of the results which would follow from such surveys as I have here advocated, I am by no means deterred by the largeness of the labours involved, even in the complete survey of the heavens with small apertures, from urging those who can do so to join actively in a work so valuable. There are so many amateur astronomers who have fine telescopes for which they can find no employment, and so many others who *do* find employment for their telescopes, but after a fashion tending in no sort to advance our knowledge, that the work ought not to languish for want of recruits. And I should imagine that no training, even, would be wanted by a large number of these recruits ; because the eagerness with which telescopists are trying to divide difficult double stars, or to see planetary features which their telescopes are just *not* able to show, and so on, seems indicative of an earnest desire to acquire a fitness for useful work. I have named certain apertures which should at once be applied to the survey, but reflectors and refractors of 5, 6, 7, or any number of inches in aperture, could be most usefully employed in sidereal survey.

* I speak here with reference to refractors. If reflectors are used in such a survey a careful comparison of their light-gathering power, and that of refractors employed in the same sort of work should be instituted beforehand. If this power were brought to an equality, or nearly so, different parts of the heavens might be simultaneously surveyed by observers using reflectors or refractors.

On a Contrivance by which it is designed to measure Time automatically, in taking Star or other Transits. By Capt. J. Herschel, R.E.

There are two ways of effecting the object of this paper. The one is to describe all the parts of the proposed mechanism, leaving it to be inferred, or subsequently explained, in what manner they answer their several purposes, and conjointly secure the end in view; the other is to start with the object to be attained, and show how the requisite conditions may be met. The last will be here followed, as offering a better chance of perfecting the contrivance, as a whole, through the substitution of other mechanical artifices than those which present themselves as at first sight sufficient.

In observing a transit, as hitherto practised, the estimation of Time is an essential element, where by "estimation" is to be understood the combined operation of the senses and the intellect. The instant of an occurrence has to be estimated, and instruction transmitted to a recorder, automatic or other, to note the same. This complex series of duties is liable to miscarriages, from which the estimation of position is free. Is it not possible to obviate this? Cannot the passage of the star's image across the wire produce the same effect directly, as that which is now produced indirectly through the medium of the observer's senses, and exertion of nervous energy? Or if this is not yet attainable, cannot the observer be enabled to throw so much of himself into the work as to cause the arrival of something *material*, coincidently with the immaterial image; and *thereby* put in action at the moment of arrival the train of forces which mark the instant? This is practicable. Let a diaphragm with a single vertical wire be driven across the field of view at the same rate as the star, and (as regards the wire) coincidently with it. In its journey let it produce the contact which completes the galvanic circuit, and impresses the time-record on the chronograph. If this event takes place at a fixed stage in the journey, it is evident that the time will always be recorded when the wire is in a fixed place, and, therefore, since the wire is to be supposed to accompany the star, when the star is in that same place. This is equivalent to determining the instant of transit over a fixed wire. And it is perfectly automatic, for the observer is not concerned with it at all, except in keeping the star and wire accurately together during their journey across the fixed point.

But how is he to do this? The only answer is, that mechanism must be provided which will enable him. By releasing a wound-up spring acting on a fan through a train of wheels, the wire diaphragm must be drawn across at any rate not exceeding that of an equatorial star. If so released when the entering star reaches the wire, the two will travel across together—not necessarily coincident, but in close proximity. Further adaptation

must enable him to restrain, or urge forward with the needful delicacy, the moving frame and wire, and so effect an intersection as though both were stationary. While one hand is attending to this, the other will hold the contact-key which is to allow or forbid the record being made, according as the observer is satisfied or the reverse with the intersection he has made, at the time of reaching the stage where contact will take place. Of course there may be any number of such stages.

The traversing mechanism is, no doubt, that which will require most ingenuity. The work which it has to do is exceedingly light, and therefore it may be made with delicacy, and with due regard to the danger of perceptible vibration imparted to the tube. Attention may also be well bestowed on that part of it which regulates its mean speed. If possible the setting of the telescope should act directly upon it, so that (within the limits of N.P.D. which experience may show to admit of its action) the speed will in every position correspond, as closely as ingenuity can make it, with that of a star towards which the tube is directed. There is ample room here for the skill of a machinist; but there does not appear to be any insuperable difficulty, since all that is really requisite is a nearly uniform motion, susceptible of moderate intentional and subsidiary acceleration, or retardation (such as may be produced by a spring acting on the circumference of a plain wheel), and subject to gross regulation by previous adjustment. An arrangement will also be necessary by which the action of the machinery on the wire frame can be thrown out of gear, so that the latter may be quickly moved to any part of the field, to pick up a star that may have entered unawares. These difficulties are not underrated; but the very great importance to astronomy of any practical means of relieving the observer from the duty of estimating time minutely seems to make them worthy of being surmounted.

There does not seem to be any corresponding difficulty in arranging the parts which are to serve the essential purpose of recording the instant of transit of the *wire*—as distinct from that of the star which it represents. An isolated needle-point projecting from the sliding-frame, and isolated hair-springs projecting from its guide frame, seem to meet all the requirements. It is, however, conceivable that the personal equation, so to speak, of the battery may vary, owing to its greater strength at one time than at another. But it will certainly be constant for the whole of each transit, and will probably be under complete control eventually. As it would be highly desirable, indeed essential for transits below the pole, or with reversed pivots, that the mechanism should permit of the wire-frame being traversed in either direction, backwards or forwards, in order that the wire may likewise accompany a retrograde object, the needle-point and springs must come into action against each other indifferently on either side. This also will cause a "personal equation" which, it may be hoped, will not be difficult of determination.

Whether the above conception is or is not susceptible of practical realization, it is difficult to believe that the method could not be made to secure, in some such way, the desired result of a mechanical determination of time. If it can, it must prove of great value to an observer of transits, by relieving him of the duty of estimating *the time* of an event.

Bangalore, September 8th, 1870.

Prof. A. S. Herschel, in a letter dated Collingwood, June 14th, 1871, addressed to Mr. Lassell, and communicating to him the foregoing paper, writes :—

In the autumn of last year my brother also sent me a paper on a contrivance for automatic registry of the Times of Transit of a star, across the wires of a transit telescope. Until the paper had been submitted to some good practical mechanical and astronomical authority, my brother did not wish it to be communicated to the Astronomical Society. I then sent the enclosed copy of it to my late father, with the small drawings 1 and 2 (following it) of the Eye-end of a Transit-telescope, showing a contrivance of a revolving sphere, driven by clockwork and driving (by rubbing) the head of a micrometer-screw. By inclining the axis of the sphere at different angles to the axis of the screw, different rates of revolution of the screw might be obtained, corresponding very nearly to the different polar distances at which star transits would be observed.

On a second page a plan of a pair of "*Marlborough wheels*" is shown, with a moveable pinion engaging them both, and capable of being made to travel round them in either direction. When fixed, it transmits the power of the clock to the telescope (or to the micrometer-screw at the eye-piece), with a fixed velocity-ratio, depending on the proportion of the number of teeth in the two Marlborough wheels. When it is moved by the hand, in one direction, it increases, and in the other direction it diminishes, the velocity ratio of the Marlborough train ; and it thus enables an observer to wait for, or to overtake, and thus to bisect a star.

The objection urged by my late father against any such construction for automatically registering the *Times* of transits, was, *that it burdens the eye-piece of a telescope* with cumbersome machinery, which is the part of the telescope that requires to be most light, and free from tremors. I also showed my brother's paper to Professor Grant, at Glasgow, who recommended its communication to the Astronomical Society. But before presenting it to you, I hoped to be able to obtain Mr. Airy's opinion of its merits, which my very close and constant occupation at Glasgow, during the last Winter Session, unfortunately prevented me from doing. I need scarcely add that my attempt to supplement the paper with a few mechanical contrivances is of no value or importance, compared to the *practical scientific interest* which attaches to my brother's original suggestions.

The drawings and explanations referred to in the letter were as follow:—

a is a bevel wheel driven with constant speed by a spindle *p* which comes down the tube of the telescope from a clock fixed upon one of the piers or pillars of the horizontal axis of the telescope. A long brass arm A turns on the axis of the wheel *a* as a centre, and carries another large bevel wheel *b* at its other end, which is driven from *a* by the intermediate pair of mitre-wheels

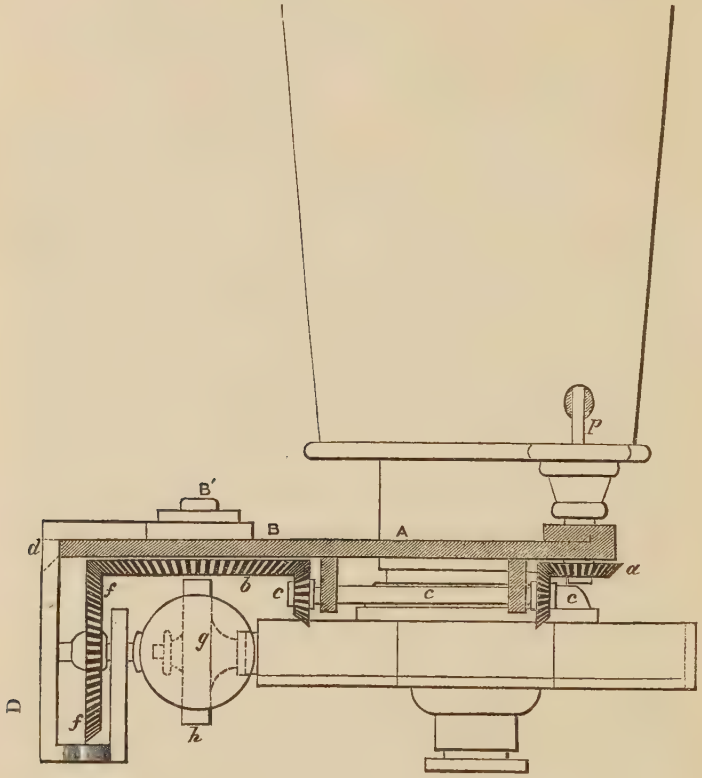


Fig. 1. View from above the Tube.

(*c c c*) on a common axis *c*, supported by the bar A. The latter ends at B in a circular plate B (a part of which is shown cut away at C, fig. 2, for lightness where it is not required); and B is pierced through its centre by the pin B′, which carries the bevel-wheel *b*, and also a brass plate, or radius, D, three times bent so as to carry the axis of another mitre-wheel, *f f*, which works in the teeth of *b*. This last axis carries the brass sphere *g*, which thus revolves with a constant speed, being wrought directly through the train of wheels, from the first clock-wheel *a*.

The sphere, *g*, rests with its whole weight, and with the weight of the lever A, and of its appendages upon the graduated head, *h*, of the micrometer, which is to be covered with an india-rubber band, and it will then be driven by the friction of the sphere. By turning the bent arm, D, round the pin B′, so as to give the axis of the sphere any degree of inclination to the vertical line, *g h*, fig. 2, corresponding to the N.P.D. of the star to be ob-

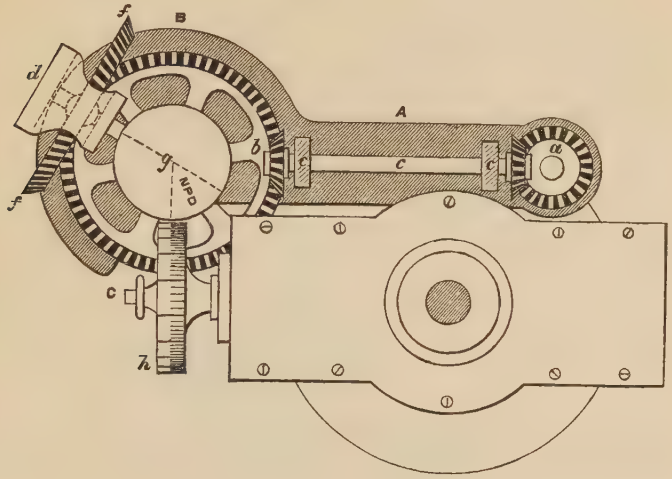


Fig. 2. Front, end view of the Tube.

served, the speed with which the sphere will drive the micrometer-screw head round, will be proportional to the speed with which the star will cross the field of view at that distance from the pole ; and to facilitate placing the axis of the sphere at its proper inclination, the edge of the circular plate, B, can be graduated, and a vernier-aperture at *d* may be cut through the bent arm, D, so as to show the N.P. Distances to the nearest degree, or 15′, for which the sphere is to be set. A little adjustment by a fine tangent-screw would bring the speed of the wire-frame into exact coincidence with that of the star after it had entered the field of view, by a few trials. And the action of a smooth sphere upon a stout band of india-rubber would probably be very regular and uniform; or might, at least, be worth a practical trial.

Instead of the weight of the lever, A, and of its appurtenances pressing upon the head of the micrometer-screw, this part of the apparatus might be made as light as possible, and might be made to press uniformly against the head of the micrometer by means of an elastic spring.

A pair of Marlborough wheels (like those fitted by Messrs. Cooke to the clock-motion of Mr. Newall's large Equatoreal

telescope at Gateshead) placed in some part of the train of wheels between the clock and the telescope (see fig. 3), would enable an observer by turning a milled head, and thus making a pinion revolve backwards or forwards round the pair of Marlborough wheels, to retard or advance the motion of the sphere, *g*, and of

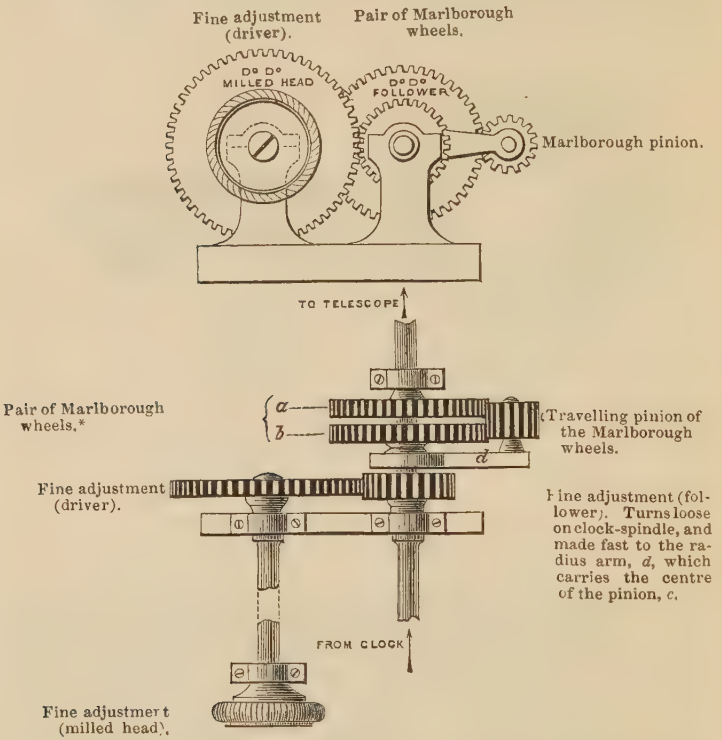

Fine adjustment (driver).
Pair of Marlborough wheels.
D° D° MILLED HEAD
D° D° FOLLOWER
Marlborough pinion.
TO TELESCOPE
Pair of Marlborough wheels.*
a
b
Travelling pinion of the Marlborough wheels.
d
Fine adjustment (driver).
Fine adjustment (follower). Turns loose on clock-spindle, and made fast to the radius arm, d, which carries the centre of the pinion, c.
FROM CLOCK
Fine adjustment (milled head).

Fig. 3. Pair of Cooke's Marlborough wheels for advancing or retarding the motion of a telescope driven by clockwork, so as to give small adjustments in R.A.

the micrometer-screw which it drives in any manner; so as to make the micrometer-wire bisect the star. When the moveable pinion is left at rest, the motion of the clock is transmitted uniformly through the rubbing-sphere, *g*, to the wire frame of the micrometer, with the proportion of its speed exactly such as to

* The wheel *a* has a few more teeth than the wheel *b*, and moves slower than the wheel *b*, when the centre of the pinion *c* is fixed. When the pinion *c* is carried forwards round the two wheels *a*, *b*, the wheel *a* is also carried forwards, or it is made to advance. When *c* is turned backwards round the wheels *a* and *b*, the wheel *a* is also turned backwards, or it is made to return. The wheel *a* has the same diameter, and is on the same line of centres with *b*; and it has *so few more* teeth than *b*, that the difference of their pitch does not prevent both of the wheels from working equally well in the pinion *c*.

keep the star bisected by the wire as long as they remain together in the field of view. The frictional bearing of the sphere upon the screw-head will prevent the clock from over-winding and injuring the thread of the micrometer-screw if it should be inadvertently left in gear with it until the screw reaches the end of its range.

The Astronomer Royal remarks on the foregoing :—

Capt. Herschel's abstract idea, that a wire should be made by mechanism to accompany the star-image, and at a definite point should make a galvanic contact, is excellent. It implies the necessity of placing the wire on the star beforehand, and of giving the wire frame a motion agreeing *very accurately* with the motion of the star-image. And here is the practical difficulty.

Professor Herschel's mechanical plan for doing this is ingenious. But it implies, 1st, an original uniform motion (not easy to get); 2nd, motion through three engrenages of bevelled wheels (which would have abundance of shakes); 3rd, adjustible motion by the surface of a sphere rubbing a drum-head at different declinations on the sphere. (I doubt whether this can be made nearly correct enough, but it is very pretty.)

I do not think that the problem is by any means solved.

Note on the Curve traversed by base-end (remotest from fixed pivot) of the last prism of a Single or Double Automatic Spectroscope. By Richard A. Proctor, B.A. (Cambridge).

Writing down somewhat in haste the equations in the Note on page 206, I failed to notice that the polar equations to the curves traversed by the points P and Q (fig. 2, p. 207) can be very readily obtained. Thus let

$$AP = r \quad \text{and} \quad \angle PAO = \theta.$$

Then θ is the component of $\angle DOA$; that is

$$\theta = \frac{\pi}{2} - 6 \sin^{-1}\left(\frac{a}{R}\right)$$

or

$$\sin\left(\frac{\pi}{12} - \frac{\theta}{6}\right) = \frac{a}{R} \qquad \text{(i)}$$

But

$$r = 2R\cos\theta;$$

hence (i) becomes

$$r \sin\left(\frac{\pi}{12} - \frac{\theta}{6}\right) = 2a\cos\theta.$$

This is the polar equation to the locus of P. It may be written

$$r\left[(\sqrt{3}-1)\cos\frac{\theta}{6} - (\sqrt{3}+1)\sin\frac{\theta}{6}\right] = 4\sqrt{2}\cdot\cos\theta.$$

The polar equation to the locus of Q is obviously

$$r \sin\left[\frac{\pi}{12} - \frac{1}{6}\left(\theta - \frac{\pi}{4}\right)\right] = 2\sqrt{2}\, a \cos\left(\theta - \frac{\pi}{4}\right);$$

or

$$r \sin\left(\frac{\pi}{8} - \frac{\theta}{6}\right) = 2\sqrt{2}\, a \cos\left(\theta - \frac{\pi}{4}\right).$$

It may be written

$$r\left[\sqrt{2 - \sqrt{2}}\cos\frac{\theta}{6} - \sqrt{2 + \sqrt{2}}\sin\frac{\theta}{6}\right] = 4a(\cos\theta - \sin\theta).$$

Occultation of 80 *Virginis by the Moon, observed at Forest Lodge, Maresfield, on May* 30, 1871. By Capt. Noble.

The star disappeared instantaneously at the Moon's dark limb

At $14^h\,25^m\,5^s$ L.S.T. = $9^h\,53^m\,13^s{\cdot}9$ L.M.T.

and reappeared at the bright limb (from behind a dome-shaped mountain)

At $15^h\,14^m\,48^s$ L.S.T. = $10^h\,42^m\,48^s{\cdot}8$ L.M.T.

These times are very good, although the atmosphere was unsteady. The power employed was 255, with my 4·2-inch Ross Equatoreal, and was adjusted on the star.

Observations of Winnecke's Comet. By the Rev. S. J. Perry.

Owing to the unusually bad weather in April and at the beginning of May, I was unable to observe this comet for a long time after its discovery. When the sky permitted of a first observation, the twilight and the nearness of the Sun rendered the object much less distinct than it had previously been, though it was still much more conspicuous than the comets of 1870.

The following positions may serve to complete the series of observations made by others more favoured by fair weather. The nearest Greenwich star ι *Aurigæ*, was chosen as the star of comparison, but an intermediate double-star was also made use of to diminish the chances of error.

	G.M.T. h m		R.A.	Decl.
May 8	10 8	ι Aurigæ	4 48 34·2	+32 57 31·4
		Dist. of Comet	− 20 37	+ 2 5 30
		Comet	4 27 57·2	35 3 1·4

	G.M.T. h m		R.A.	Decl.
May 8	9 57	ι Aurigæ	4 48 34·2	32 57 31·3
		Dist. of Comet	−17 36	+1 22 35
		Comet	4 30 58·2	34 20 6·3
10	9 43	ι Aurigæ	4 48 34·2	32 57 31·3
		Dist. of Comet	−14 2·	+37 40
		Comet	4 34 12·2	33 35 11·3

Observations were also taken on the 11th and 12th, but the nearness of the comet to the horizon and the passing clouds rendered the results less trustworthy.

Stonyhurst College Observatory,
June 6, 1871.

On the Total Solar Eclipse of 1871, *December* 11–12, *on the Australian Continent.* By J. R. Hind, F.R.S.

The total eclipse of the Sun in December next would be most advantageously observed on the northern coast of Australia. I am not aware, however, that the track of country situated within the zone of totality is anywhere colonised, and to secure observations it would probably be necessary to send round observers from Sydney or Melbourne.

To afford an idea of the circumstances of the eclipse in this region, I have selected four points near the coast from the Admiralty map of North Australia, and have made special calculations for these points, taking the elements of the eclipse from the *Nautical Almanac.*

The positions selected are—

	Long.	Lat.
	° ′	° ′
Vansittart Bay (S. extremity)	126 18 E.	14 10 S.
Mount Casuarina	127 37	14 23
Pearce Point	129 22	14 27
Groote Eylandt (S. E. extremity)	137 2	14 15
Cape Sidmouth	143 38	13 25

The following are my results:—

		Totality Begins.	Totality Ends.	Duration of Totality.	Sun's Alt.
		h m s	h m s	m s	°
Vansittart Bay	Dec. 12	0 48 9·3	0 52 16·7	4 7·4	74
Mount Casuarina	,,	0 56 57·6	1 1 13·5	4 15·9	72
Pearce Point	,,	1 9 1·3	1 13 13·1	4 11·8	70
Groote Eylandt	,,	2 0 16·2	2 3 59·5	3 43·3	59
Cape Sidmouth	,,	2 41 37·2	2 45 3·9	3 26·7	49

The first and last contacts occur as below :—

		First Contact. h m		Last Contact. h m
Vansittart Bay	Dec. 11	23 15	Dec. 12	2 25
Mount Casuarina	,,	23 24	,,	2 34
Pearce Point	,,	23 37	,,	2 45
Groote Eylandt	Dec. 12	0 33	,,	3 31
Cape Sidmouth	,,	1 20	,,	4 7

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